to 1,000 or 1,500 for a single field of his microscope. He then varied the mode of infection. He inoculated healthy worms with the corpusculous matter, and watched the consequent growth of the disease. He showed how the worms inoculate each other by the infliction of visible wounds with their "crochets." In various cases he washed the "crochets," and found corpuscles in the water. He demonstrated the spread of infection by the simple association of healthy and diseased worms. In fact, the diseased worms sullied the leaves by their dejections, they also used their crochets, and spread infection in both ways. It was no hypothetical infected medium that killed the worms, but a definitely-organised and isolated thing. He examined the question of contagion at a distance, and demonstrated its existence. In fact, as might be expected from Pasteur's antecedents, the investigation was exhaustive, the skill and beauty of his manipulation finding fitting correlatives in the strength and clearness of his thought.

Pébrine was an enigma prior to the experiments of Pasteur. "Place," he says, "the most skilful educator, even the most expert microscopist, in presence of large educations which present the symptoms described in our experiments; his judgment will necessarily be erroneous if he confines himself to the knowledge which preceded my researches. The worms will not present to him the slightest spot of pébrine; the microscope will not reveal the existence of corpuscles; the mortality of the worms will be null or insignificant; and the cocoons leave nothing to be desired. Our observer would, therefore, conclude without hesitation that the eggs produced will be good for incubation. The truth is, on the contrary, that all the worms of these fine crops have been poisoned; that from the beginning they carried in them the germ of the malady; ready to multiply itself beyond measure in the chrysalides and the moths, thence to pass into the eggs and smite with sterility the next generation. And what is the first cause of the evil concealed under so deceitful an exterior? In our experiments we can, so to speak, touch it with our fingers. It is entirely the effect of a single corpusculous repast; an effect more or less prompt according to the epoch of life of the worm that has eaten the poisoned food."

It was work like this that I had in view when, in a lecture which has brought me much well-meant chastisement from a certain class of medical men, and much gratifying encouragement from a different class, I dwelt on the necessity of experiments of physical exactitude in testing medical theories. It is work like this which might be offered as a model to the physicians of England, many indeed of whom are pursuing with characteristic skill and energy the course marked out for them by this distinguished master. Prior to Pasteur, the most diverse and contradictory opinions were entertained as to the contagious character of pébrine; some stoutly affirmed it, others as stoutly denied it. But on one point all were agreed. "They believed in the existence of a deleterious medium, rendered epidemic by some occult and mysterious influence, to which was attributed the cause of the malady." Between such notions and the work of Pasteur, no physically-minded man will, I apprehend, hesitate in his choice.

Pasteur describes in detail his method of securing healthy eggs, which is nothing less than a mode of restoring to France her ancient prosperity in silk husbandry. And the justification of his work is to be found in the reports which reached him of the application, and the unparalleled success of his method, at the time he was putting his researches together for final publication. In France and Italy his method has been pursued with the most surprising results. It was an up-hill fight which led to this triumph, but it is consoling to think that even the stupidities of men may be converted into elements of growth and progress. Opposition stimulated Pasteur, and thus, without meaning it, did good service. "Ever," he says, "since the commencement of these researches, I have been exposed to the most obstinate and unjust contradictions; but I have made it a duty to leave no trace of these contests in this book." I have met with only a single allusion to the question of spontaneous generation in M. Pasteur's work. In reference to the advantage of rearing worms in an isolated island like Corsica, he says:—"Rien ne serait plus facile que d'éloigner, pour ainsi dire, d'une manière absolue la maladie des corpuscles. Il est au pouvoir de l'homme de faire disparaître de la surface du globe les maladies parasitaires, si, comme c'est ma conviction, la doctrine des générations spontanées est une chimère." It is much to be desired that some really competent person in England should rescue the public mind from the confusion now prevalent regarding this question.

M. Pasteur has investigated a second disease, called in France *flacherie*, which has co-existed with pébrine, but which is quite distinct from it. Enough, I trust, has been said to send the reader interested in these questions to the original volumes for further information. I report with deep regret the serious illness of M. Pasteur; an illness brought on by the labours of which I have tried to give some account. The letter which accompanied his volumes ends thus:—"Permettez-moi de terminer ces quelques lignes que je dois dicter, vaincu que je suis par la maladie, en vous faisant observer que vous rendiez service aux Colonies de la Grande Bretagne en repandant la connaissance de ce livre, et des principes que j'établis touchant la maladie des vers à soie. Beaucoup de ces colonies pourraient cultiver le mûrier avec succès, et en jetant les yeux sur mon ouvrage vous vous convaincrez aisement qu'il est facile aujourdhui, non seulement d'éloigner la maladie régnante, mais en outre de donner aux récoltes de la soie une prospérité qu'elles n'ont jamais eue.'

Royal Institution, 30th June JOHN TYNDALL

WHAT IS ENERGY?

III.

THE CONSERVATION OF ENERGY

IT is well-known that certain organisms of the animal world do not confine themselves to one state of being or to one order of existence, and the most familiar instance of this roving habit of life is the caterpillar, which passes first into the chrysalis state, and after that into the butterfly. This habit is not, however, peculiar to the organic world, for energy delights in similar transmutations, and we have just seen how the eminently silent and invisible electrical current may occasionally be transmuted into the vivid, instantaneous, awe-inspiring flash of lightning. Nor is this element of change confined to our peculiar corner of the universe, but it extends itself to

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tissues does not imply also the redevelopment of the insect. That the tissues are all so redeveloped is undoubted, but they are not all redeveloped at once. I have stated in my book again and again that certain organs are redeveloped in a particular manner, and was never under the impression that the whole was a case of alternate generation. I did not know the origin of the imaginal discs in those days.

With your permission I will add a few words in support of the assertion "that the pupa change is analogous to ordinary ecdysis, of which it is a modification." In ordinary ecdysis the muscles undergo degeneration at their points of attachment to the cast skin; in metamorphosis this change is far more marked. In ecdysis in Chloëon, for instance, Sir J. Lubbock (Linn. Soc. Trans., vol. xxiv.) has shown that the wings and thorax are gradually developed through nine successive sheddings of the skin. In the more remarkable metamorphosis of Lepidoptera they are developed in two ecdyses, these two being called metamorphosis. Prof. Owen believed, and the assertion is now widely known, that the larvæ of such insects as the Orthoptera, Neuroptera, &c., exist in the maggot form in the egg; but the observations of Mr. Newport on Meloe, and of Fritz Müller, of Weismann, and many others, go far to prove that this is not so—that the maggot form is intermediate, the half-developed embryo and the pupa or perfect insect, being most alike.

The subject is one of great interest, and therefore I trust you will excuse this long trespass on your pages.

99, Guilford Street BENJAMIN T. LOWNE

In Re Fungi

YOUR sarcastic correspondent "F. L. S." is quite incompetent to reply to my former letter. I did not call in question the correctness of the determination of *Agaricus cartilagineus*, but merely drew attention to the absurdity of the statement that the said determination was made from a mere "mass of mycelium," and that such a statement should come from a journal specially devoted to Botany.

In the original report of the occurrence of *Agaricus cartilagineus* (*Journal of Botany*, vol. iii. p. 28) special reference is there made to the "many-headed pileus;" now some of these "pilei" (not the "mycelium," "F. L. S.,") were forwarded to the Rev. M. J. Berkeley for examination, and from *these materials* he (and not the writer of these lines) made out the plant to be *A. cartilagineus*. Certainly I included the species "without hesitation" in the list of Middlesex Fungi, because I knew the plant referred to had not been determined from a mere "mass of mycelium," but that Mr. Berkeley had examined the perfected parts.

I fail to see why "F. L. S." is so anxious to "allay my alarm as to the decay of Fungology in England," especially as I have never expressed any "alarm" on that head. I do not look upon the *Journal* as such an infallible weathercock as to connect its wrong statement with a national breakdown in Botany; neither do I see how I have "missed the point" of its paragraph. I am more inclined to think that I have *hit it* in a friendly way, and rather hard too. W. G. S.

Mr. Baily on Kiltorkan Fossils

IN your last number Mr. Baily is said to have brought forward at a meeting of the Geological Society of Dublin "some strong facts to prove that the Irish palæontologists had not misled Prof. Heer, as stated by Mr. Carruthers at a recent meeting of the London Geological Society."

At the meeting referred to, Prof. Heer placed the Irish beds at the base of the Carboniferous series, mainly because *Sagenaria Veltheimiana*, a coal measure plant, was found in them.

Into this error I said "Prof. Heer had been led chiefly by the erroneous determination of the Kiltorkan *Lepidodendron* by the Irish palæontologists." I will not burthen your columns with the strange history of the nomenclature of this plant, as I shall have an opportunity of doing this elsewhere ere long. The point before us is this, that Mr. Baily alone has the credit of erroneously determining the Kiltorkan plant to be the same as an already described Carboniferous species. And the proof of this is easily adduced. In 1864, Mr. Baily, in his "Explanation of Sheets 187, &c., of the Irish Survey," figures the fossil, and describes it unhesitatingly as "*Sagenaria Veltheimiana*, Sternb. sp." This he repeated in a paper by the lamented Prof. Jukes in 1866 (*Journ. Geol. Soc. Ireland*, i. pp. 123, 124), as well as in a paper by himself read to the Natural History Society of Dublin in the same year (p. 2). Prof. Heer acknowledged his obligations to Mr. Baily for the Irish specimens he had examined. I have examined specimens so distributed by Mr. Baily, and they were named *Sagenaria Veltheimiana*.

In the volume of the British Association Reports, published in 1869, Mr. Baily says (p. 59) that the *Sagenaria* is named by Schimper *S. Bailyana*. More recently (Nov. 1871), in his "Figures of British Fossils" (p. 84), he names it *Knorria Bailyana*. It is not much to the purpose to say that it is neither a *Knorria* nor a *Sagenaria*, or further that the specific designation *Bailyana* must give place, with some dozen other synonyms, to the original name given by Dr. Haughton in 1855. But it is to the purpose to notice that *Sagenaria Veltheimiana* is *not* a Kiltorkan fossil, though said to be so by Mr. Baily, and that this error, now acknowledged by Mr. Baily himself, was the main foundation of Prof. Heer's argument.

I am not a little curious to know what are the "strong facts" which will overthrow a plain narrative that fully justifies my statement, but at the same time compels me to make it more personal than the truth seemed to me to demand when I made it some months ago. WILLIAM CARRUTHERS

ZOOLOGICAL RESULTS OF THE ECLIPSE EXPEDITION

A STEAMER is eminently unqualified for observations on marine zoology. Owing to the high rate of speed, it is impossible to use a towing net with any success, and to a zoologist it is perfectly tantalising to see swarms of Medusæ, &c., sail past the ship without being able to obtain a single specimen. In Peninsular and Oriental ships the only practicable method is to keep the tap of the baths constantly running through a fine gauze net. In this way quantities of Entomostraca may be obtained. Since we have been in the Red Sea, the water has been splendidly phosphorescent every night, the light being most brilliant where the hot water from the condensers is shed out into the sea, the animals being probably killed by the heat, and emitting in the act one last brilliant flash. If the water be turned on into one of the baths at night, most gorgeous flashes of light are obtained, and the animals causing them may be caught in small vessels and kept for examination. They are at present almost exclusively Entomostraca of the genera *Cypris*, *Cyclops*, and *Daphnis*. When the light is examined spectroscopically, it gives a spectrum in which only the green and yellow are present, the red and blue being sharply cut off. Several species of the Entomostraca obtained contain a brilliant red pigment, which gives unfortunately no absorption bands when examined with the micro-spectroscope. At Suez I obtained a number of Echinodermata of the usual dark purple tint, a splendid *Comatula* in abundance, two species of *Echinus*, and one or two star-fishes. The colouring matter of these animals is readily soluble in fresh water or alcohol, as is that of the common British feather-star. Though its colour is extremely intense, it gives no absorption bands, but when a strong solution is used, the spectrum is reduced to a red band, all the rest of the light being absorbed. Apparently parasitic on a large flat *Spatangus*, were obtained a number of *red* Planarians, about one-eighth inch long, which gave the characteristic absorption bands of hæmoglobin with great intensity. The existence of hæmoglobin in Planarians is a fact of considerable interest, and I believe quite new. On taking a boat excursion round the shores, where I obtained abundance of large Gasteropods and the Echinodermata mentioned above, I was remarkably struck by the absence of Actinias. Though I was out nearly the whole day, I did not see a single specimen, nor indeed did I observe any large Medusæ. This absence of these latter may perhaps, however, have been due to the set of the wind or tide.

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